

# Kornberg, Arthur 2003

## Dr. Arthur Kornberg Oral History 2003

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Arthur Kornberg Oral History  
Interview

October 31, 2003

This is an interview with Dr. Arthur Kornberg at Stanford University. The interviewer is Dr. Buhm Soon Park of NIH History Office, and today is October 31, 2003. The subject of our discussion is about Dr. Arthur Kornberg's experiences as an intramural researcher at NIH and his view on NIH in general.

Buhm Soon Park: Well, thank you very much, Dr. Kornberg, for giving me this opportunity, wonderful opportunity to talk to you. As I told you, it seems to me that we can discuss a number of subjects in the history of biochemistry of the 20<sup>th</sup> century because of you were one of the leading figures who shaped the biomedical field. But our time is limited and my goal is on the history of NIH. But if at any time, if you want to talk of that, I would be happy to listen. But at the moment, our focus is on the history of NIH and your experience at NIH as an intramural researcher and your experience as an outsider and supporter of NIH. Now, shall we start with the question of how you came to NIH in the first place?

Arthur Kornberg: Well, I'll try to answer these with a brevity that assumes you're looking at some materials that I've written; for example, the last lecture, the symposium. And in it, I borrow on what Lederberg, Joshua Lederberg, wrote in the forward for my book, *For the Love of Enzymes* -- very well written, and generally, I'm pleased with the book itself -- on how people came to their careers in science, and particularly medical, biomedical science, and he points out that people like himself and Tabor early on knew about research, medical science, and more or less directed their careers that way; unlike the other group, like myself, that really didn't know about research or science, and in some haphazard way was thrust into it and then, with some kind of epiphany, sense of discovery, said, "Hey, this is it! This is what I want to do." And so we've each found our way, and in my case, one could analyze it much farther back. But let me say simply that if it hadn't been for the accident of Pearl Harbor, I don't know what led me to choose the Public Health Service, but if it were important, one could delve anyway. And then Leon Heppel was then persuaded by me, even though my chairman of medicine, when I was an intern, wanted me to join the Navy or the army. And through the Public Health Service, which was in the Department of the Treasury, and then was responsible for the Coast Guard, and I was assigned to the Coast Guard, and I did interviews in Boston for Coast Guard recruits, and I did the medical examinations for a month or so, and I was assigned to a ship. And the several months I spent on a ship -- I don't know many, maybe three months; the most wonderful vacation I've ever had in my life. Now, the ship had its functions. It was trained merchant seamen in gunnery and other posts that they would have on convoy fleets. But it ran between St. Petersburg and Key West and the Gulf of Mexico, and I guess there were risks at that time from submarines, but it really didn't affect me. And so, what else did I do on the ship? As a student, I was informed by people who observed me that I had jaundice, yellow sclera -- you know; the whites of your eyes get a little yellow. I've written about this in my book, so I don't want to go into too much depth. But anyway, through a series of events like that, as I look back on it, I was interested in collecting data, even if it was very difficult to do so, even though I wasn't supported in it. So I think the germs of that do go back. And I published a paper, which, in a very prestigious journal, the *Journal of Clinical Investigation*, and at that time, recited the experience that I had with myself, a few others, of having this jaundice, of unknown origin, not realizing at the time -- this is important -- nor did the editors of the journal or the staff of the Department of Medicine, there was something called genetics, that some diseases had genetic origins. Now, of course, it was known that some things like hemophilia was hereditary genetically, but this genetic defect of not being able to remove bilirubin from the blood, how uncommon, a few percent of the population had it, and realized around 1900, a Frenchman by the name of Gilbert had described it, totally forgotten for years. And so the -- this is an aside -- the lack of attention given in biology or medicine to genetics was apparent in this case. Anyway, the fact that the title of that paper mentioned jaundice, and the fact that at that time, troops going to the Far East were given yellow fever vaccine that was loaded with hepatitis viruses for some of them, and led to jaundice, attracted some attention, and the director of the NIH, Rolla Dyer -- a very nice man, infectious disease person. NIH was an infectious disease laboratory.

Park: Mostly, yes.

Kornberg: With a few exceptions, some nests here and there. And Leon Heppel, who had a Ph.D., went directly to NIH, campaigned repeatedly. NIH was very small. He had lunch with the director every day or frequently, and said, "You know, Arthur Kornberg should . . ." So eventually, there was an opening. There were very few openings at the NIH. He said one day, "You know, we have an opening. What about that Swedish friend of yours?" Now, Kornberg here is a Jewish name, and I think there was some residual or sustained antisemitism . . .

Park: At NIH.

Kornberg: NIH, as elsewhere. That Swedish . . . I don't know. I never presumed. Anyway, I was then transferred to the NIH. Just a tour of duty. And I was assigned to the nutrition section and completed that detail.

Park: Sebrell was there?

Kornberg: Sebrell, as I mentioned in Bethesda a couple of weeks ago, was an NIH -- I'm sorry -- a Public Health Service regular commissioned officer, and how he got to Goldberger's laboratory, I don't know, but I think you should find out.

Park: Yes.

Kornberg: Since it is the period you're interested in.

Park: Right.

Kornberg: But anyway, he was chief of the section, division that included nutrition, pharmacology. Again, you'd have to look. There was Rosenthal, and then Tabor, who came and joined, was in that division. But I was there just for a tour of duty. I wasn't supposed to be there. But in a short time -- and this is where I think I was distinguished from even Heppel, who felt they were now being diverted from their careers, clinical careers, and kept up with clinical literature so that they wouldn't be utterly rusty. And within a year, having assumed I would be doing clinical medicine, I just decided it was more interesting to work on rats, controls and so forth. Again, I had gone into that. So I would say my personal leanings or attributes or whatever, unlike Tabor or Heppel and many others, was to do something decisive, giving up clinical medicine. After a few years of feeding rats, I was bored with it. I heard about enzymes and then left, Herb Tabor thought I was a little crazy doing that. A thriving enterprise that was producing lots of papers and so forth.

Park: Who influenced you to study more enzymes?

Kornberg: Well, we had this group of four: Horecker, Heppel, we were reading these papers by Lipmann and Kalckar and others. I thought that was really great. I heard about the Beadle's work, one gene, one enzyme. These were very novel things, and I had the sense that what I was doing was rather old-fashioned, maybe primitive in terms of the future. I didn't know what biochemistry was.

Park: But you didn't have any difficulty in reading and analyzing the literature.

Kornberg: Well, the literature was limited. The biochemistry came out once a month. Horecker had a Ph.D. in biochemistry and Heppel got a Ph.D. in physiology, with some understanding. Tabor was at that time interested in electrolytes. I would say I was the least trained of the group by far. But we were a very intimate, congenial group, and we presented papers every noon, went through them in some detail. But still, when I came to Ochoa's group, very little. And then when I came to Washington University after a year with Ochoa, Mildred Cohen -- you know her name? Mildred celebrated her 90<sup>th</sup> birthday last year and became very up-front. She said, "You know, I was astonished at how ignorant you were." The gaps of my knowledge of basic biochemistry, enormous.

Park: You were talking about your lack of training in biochemistry when you went to the University of Washington.

Kornberg: Washington University.

Park: Washington University at St. Louis.

Kornberg: And Mildred Cohen saying she couldn't believe how ignorant I was. So my -- I didn't have a proper postdoc and didn't have the courses. But I don't think that's that important. I had an interview with a group just before you came, establishing the Okinawa Institute, and pointing out to them that this just came in, the reprint. Have you seen that?

Park: No.

Kornberg: So, among the Ten Commandments, which I've now amended, rely on enzymology. Thou shalt rely on enzymology to resolve and reconstitute biologic events. So my dedication to enzymology as a way to solve, begin to understand biologic problems is a focus that's not widely shared. Now it's genomics or proteomics and all that stuff. It doesn't include enzymology. And at the Okinawa Institute, everything is multidisciplinary and all of these directions. Some of them are obviously very fruitful, but to the exclusion of enzymology. And once I became interested in biochemistry, commandment two is "Trust the universality of biochemistry and the power of microbiology." Again, no one taught me, but I realized, working on rats isn't going to get you anywhere. It may be better than people, but . . . And then, working even on yeast is complicated and slow. But working on *E. coli* and now other bacteria are much closer to enzymes as sources, means of . . . And now, with genetics, of course, the power of genetics is such that you don't have to rely on inhibitors. We can check the significance and the generality of an enzyme event by removing it, modifying it.

Park: So, Ochoa was at New York University and was the person that you could find in the United States to work with at the time?

Kornberg: I could find others, but I had the benefit of having advice from Bernie Horecker and others that he had no one else working with him, and exceedingly able, doing something that many people were focusing on, but he was doing well. The source of ATP. How is ATP made? And a really wonderful example of focus on science despite all kinds of difficulties from the environment: civil war in Spain, war in Germany, war in England, not having a laboratory in the U.S. His capacity to focus I think is one of the greatest attributes of science, not to be distracted, just to keep doing something with the confidence, if you work at it, one way or another, serendipitous or directed, you will make progress in some direction.

Park: How was that fellowship arranged?

Kornberg: It was not a fellowship.

Park: It wasn't?

Kornberg: No. I was assigned. Sebrell used to say, you know, we've run out of vitamins. These enzymes, I don't know what they are, but people are discovering new enzymes. And Floyd Daft -- do you know his name?

Park: Yes.

Kornberg: Who is really -- I think he was trained or got a degree in biochemistry at one level or another. Very bright. I haven't seen his name mentioned. But he was directed by Sebrell officially. Sebrell had put his name on all my papers. Daft did only when we did something together. But smart. And I'm sure he helped advise Sebrell, bright young man; let him do that thing -- maybe for three months, stretched out to a year, arrange to go to the Corey laboratory another six months. Very generous. I was in uniform. I was just, where, if I went, I was the commanding officer of the base in New York, at New York University. So the NIH -- it's not a policy. I don't know anyone else who . . . Maybe someone, another person. It was really wonderful that that government agency supported me in a new career, really remarkable.

Park: Wasn't there anybody else who wanted to follow your suit?

Kornberg: I don't know. I don't know that there were any precedents, I don't know that there were any subsequent people. I have no idea. But in this particular instance, Sebrell, Daft, and others. I don't know whether they had to get permission from the director of the NIH. He subsequently became a director.

Park: Yes. When you came back to NIH . . .

Kornberg: Oh, this is funny. I haven't told this anywhere. I came back to the NIH, and then -- my memory may be inaccurate, but there was an incident, a moment, when I came back, and Sebrell and Daft had already arranged for me to have a laboratory in Building 3. It was not evident there was any space in Building 4, where I'd been. And so they arranged between themselves to have this practical joke, and I asked, "Where will I work?" and they showed me to some little closet or nook on the fourth floor, or the third floor of Building 4. I said, "Really?" I said, "Well, I'm leaving." I had no place to go, but I say, "This won't do." And, "No, we're just kidding. We have this place," and also set up a section, which I was named head of. And at that moment, within weeks, Horecker and Heppel's place in Building 2, which was industrial hygiene, they were going to be sent to Cincinnati or somewhere -- I don't remember -- because the industrial hygiene mission was over. And so I arranged in some way -- I can't remember -- to have them join this little section of, whatever it was called. And so there were three of us now at this place on the first floor of Building 3.

Park: It was quite interesting to see that here the researchers trying to get together and do the research. Was it possible, you know? NIH has its own bureaucracy and administrative levels, and probably somebody up high decided just to get scientist watching. But back then, was it quite possible for you, and Leo Heppel?

Kornberg: Well, possible? Yes, it happened. And how much, how novel was it?

Park: Right.

Kornberg: How much maneuvering did it take? Who twisted whose arm? I don't know. I think the authority rested with Sebrell, and he had enough authority to somehow appropriate or get access to the space in Building 3. I don't know what was going on in Building 3. They had malaria research and a lot of shuffling and reshuffling. NIH was a small place, very small place.

Park: And when you came to Building 3, well, right after that, did you have postdoctoral fellows or somebody else coming in? At some point, I saw a flood of postdocs from all over the place to work with you.

Kornberg: Well, I came there in the fall of '47, and I think my first -- we have to go back to the records. Osamu Hayaishi who has been the most prominent bio scientist in Japan, he came to do a postdoc with me, and that's another history. I don't know if we should go into it. Very impressive, and very few postdocs. Each had one or two. But I mentioned in the Taylor symposium, and here the historical record is not, the data are not available, but my recollection is that in mid-'52, I had applications that I had accepted from four people who would be major stars in biochemistry: Paul Berg, Bruce Ames, Gordon Tompkins, and Ed Korn. And at that time, for reasons we could go into, I decided I didn't want to stay at the NIH any longer, because I had this very attractive offer -- not so attractive, as it turned out -- from Washington University at St. Louis, a very prestigious place, and when I told each of the four that I was moving, Bruce Ames was then co-opted by somebody, Horecker, who, of course, remains in Building 3. Ed Korn told me -- again, it was complicated, Gordon Tompkins, whom I obviously can't discuss this with. Only Paul Berg, who was a young star -- he had been directed by Harland Wood to spend a year in Copenhagen with Kalckar and a year in the Corey lab. And Paul said, "I don't want to go to St. Louis. I'll work with Arthur Kornberg in..." I now decided to go to St. Louis. And, fortunately, I've been.... He was just on the phone. He came to St. Louis and, typically, did so many important things, even as a postdoc. So when I look at why did I leave the NIH -- and now we're the victims of selective memory, aren't we -- number one, I saw a clinical center going up, and I said, you know, this place and their diseases..., and I was advanced, sophisticated enough, to have a concern that clinical research would degrade the quality and the freedom of doing basic research, gigantic clinical center. The directors of the Institute were uninspiring people, largely bureaucrats of one kind or another. I told a story to someone I'd heard saying to someone else. One Sunday or a holiday, we're going to have a picnic, and I needed an ice bucket. At that time, we didn't have these foam-rubber, nicer buckets. I had built, I'm sure, one pail nested in another with some insulation. That was my ice bucket. This was the key element of doing biochemistry, put all of your tubes in the bucket of ice, and then you . . . It was a miniature cold room, of course. I filled the bucket with ice and I was walking out the door, and an armed guard said, "Where are you going with that bucket?" "I'm going on a picnic." He said, "You can't take that government property out." You know, there was no reasoning with him. I knew immediately. He was there, he was a guard, had a gun. He wasn't going to do . . . You know, that's an indignity that I think I don't want to live with. No one would question my taking a bucket out of Washington University microbiology department.

Park: Were you the only one who felt that way? That the clinical center . . .

Kornberg: I don't know. It would be important for you to find out how much of a threat it imposed. But what I've said repeatedly, through very wise direction, Shannon must be -- and who else? I don't know -- was able to exploit the public's support of research in disease, constituencies, multiple institute of the month. What was it going to be? But that was exploited. And even though Congress, then and now, says, "What are you going to do this year? Give me the program," they, Shannon and others, had a long-range vision. The cancer research didn't have to be on cancer-bearing animals. They could be in cell cultures. In a variety of ways that you have to look into, I think, NIH, its intramural program, and then its extramural program, was able to interpret to congressional committees that this was basic information needed to do clinical work. That's very important. I don't know how it's done step by step, but congressional hearings, testimony, whatnot. I don't know how much of that you're going to do. It's going to be tough, but along the line, people testified before key committees, Appropriations Committee, whatever, and were able to persuade Congress that this work that had nothing directly to do with polio or cancer or heart disease . . . There was also an atmosphere in the country -- just talking about Yanofsky, Charlie Yanofsky working on tryptophane biosynthesis, *e. coli*, was given the American Heart Association professorship, one of the great gems. The times then, I think, must have been much more permissive and broad and understanding. How could Yanofsky, working with *coli*, get an American Heart Association professorship? So when you analyze how the NIH came to be the unique institution that it is, although there were other features of the culture, I don't know what they were.

Park: Certainly the '50s, right after World War II, there was a consensus.

Kornberg: A Sputnik kind of . . .

Park: People, Sputnik through World War II, the general public became aware of the power of basic science, with the atomic bomb and hydrogen bomb and penicillin, and its, you know, immediate impacts of basic research, which had been done without the dream of applying that to the weapons and other things. So people had a general perception to . . .

Kornberg: You're saying that, but why should I believe you? I don't know. Was there an instrument that measured public support of basic science? Whatever it is and the shifts of opinion, and science is very fashionable. You can't -- I think I've said this in several places -- *e. coli* was a powerful instrument. It's already shown how transcription works, translation, fundamentals of recombination, repair. You can't work on *e. coli*. You won't get support from the NIH. A few labs. I'll get it, a few others, but you don't dare apply for a grant based on *e. coli*.

Park: These days?

Kornberg: These days.

Park: How about yeast?

Kornberg: Yeast? Maybe you can get away with yeast. You know, yeast is very complicated. Of course, worms and drosophila and so forth.

Park: When do you think that shift happened?

Kornberg: I don't know, but it'll be within the period that you're studying.

Park: Right.

Kornberg: You've got a big job.

Park: Let me go back a bit, back to the '50s, because you mentioned Jim Shannon, and Jim Shannon was the associate -- the first job was associate director of the Heart Institute.

Kornberg: Yeah. You've done a nice piece in your paper.

Park: He would have interruption with the Heart Institute school, like Chris Anfinsen and Julie Axelrod.

Kornberg: I don't know whether I've described this. We had this, at the end, far end of the first floor, we had several labs, and there was one room, a module, we called them, 10 x 20, 10 feet wide. I don't know what was in there. Something. And one day I saw this rather handsome figure with a coat with velvet lapels and maybe a homburg or a fedora walking in my direction, and I tried to escape. I thought it was a salesman or something. And later, someone said, "There's someone to see you," and I don't want to see him, but his name is Anfinsen and I met Chris Anfinsen for the first time. And he was now designated by Shannon to head up whatever the unit was called. And he was persuading very bright young people -- I didn't know who they were, the Stadtman's and Steinberg and several others -- and he says, "You know, we don't have any place to put these people." I said, "You know what? Why don't we clear out that module and you can have that space." Did you hear the story?

Park: No.

Kornberg: It may be different. So I said, "Okay." Stadtman and a few others, virtually, it must have been a dozen people milling around in this little space. You know, I'd walk down and I'd be embarrassed. I wouldn't even want to look in there. But anyway, it kept them out of the cold. I don't know what happened. But that was 1950. And eventually something happened and they then had proper laboratories. But I think you did describe it in your paper, that Shannon's capacity to identify talented people and support basic research goes back. I didn't know him, really, by '52, when I left.

Park: Wasn't there any counteroffer for you when you decided to move to St. Louis from NIH? Something like, "Don't go; I'll add some more space."

Kornberg: I don't remember. It could have been. What I did here was that I was nine years away from retirement. I mean, I put in 10 or 11 years as a commissioned officer. In nine years, you'll be able to retire. That was the most compelling argument against staying. But I was stupid because I went to Washington University. But there was no accounting for all the years I'd put in without contributing to a pension or retirement. Salary was really overly modest. Somehow it didn't matter. And I was very fortunate that my wife -- I just can't say enough about her understanding, her dedication to science, what I was doing, and relative indifference to practical matters. It's important, too, in science that you have an environment, domestic or beyond, that helps to cope with all the turbulence and other things.

Park: Your wife was also a scientist?

Kornberg: She was a biochemist before I was. And she should have had a Ph.D. at Rochester, but didn't pursue it. She then was employed at the National Cancer Institute. I had met her before, but chiefly there in Bethesda. And she later worked in my lab, and really devoted to learning things, not to be entrepreneurial or have a lab or get publication.

Park: Did she continue to work in the lab when you moved to St. Louis?

Kornberg: Oh, yes.

Park: Even at Stanford?

Kornberg: And briefly at Stanford. And then, you know, with the progress and the fragmentation of science, genetics, and other things that came in, I think she found that kind of competition and turbulence, and by then our children were of an age where it mattered more that we were there. As you know, two of my sons are prominent scientists, and a third one is, I would say, the leading architect in the design of laboratories. So without really any kind of direction except indirect that science was a very happy way to spend a life. I don't think it mattered. My second son was a musician. He was at Juilliard. He was a cellist and he hurt his finger, and one thing led to another. But science then came to him late, but an outstanding developmental biologist.

Park: In your book and in other places, you mentioned that you loved to go to the lab on weekends and holidays and evenings.

Kornberg: Yeah.

Park: Was it well excused by your wife?

Kornberg: Well, but we were just discussing it the other day because now our labs, which this department is still one of the esteemed departments and attracts very, very good people, nobody here at night. People aren't here on weekends. Then I checked with others, and somehow the culture has changed.

Park: Yes. It's changed.

Kornberg: But when I was at Washington U or NIH or here at Stanford, the place was buzzing at night and weekends. But anyway, the ease of collecting good data, that's changed, I think. You don't have to work as hard.

Park: Because of interns?

Kornberg: I don't know. Again, these are cultural changes that -- I can speculate a little here. I think being a historian and trying to get the threads of all of these features -- economic, political, cultural resources, of national influence, as we discussed -- I wouldn't know how to put it together.

Park: Yeah. It's a lot.

Kornberg: But let me get to a point that I think you raised, and I want to be sure. The NIH, its greatness, uniqueness, is the extramural program. Don't let's deny that. And I also want to mention -- and I've written about this -- if you want a control of what the NIH did that was unique, look at the Department of Agriculture. The laboratories of agricultural experiment stations, federal and state, that was the chief research arm of bioscience before then. NIH had infectious disease laboratories, but it was minor. And so what happened during and after the war? The Department of Agriculture remained the same. Recombinant DNA and its influence on livestock and crops -- that came from the NIH.

Park: That's true.

Kornberg: The Department of Agriculture remained in the classic mode, and contrast what happened to the hygienic laboratory and the extramural program in the Department of Agriculture and its laboratories and extramural program.

Park: So when you moved to St. Louis, did you apply for NIH grant right away?

Kornberg: Oh, yes, and I got an NSF grant.

Park: NSF grant.

Kornberg: I must have had NIH grants, too. \_\_\_\_\_ but they were adequate. My lab was small. I \_\_\_\_\_ five people \_\_\_\_\_.

Park: Was housekeeping difficult, I mean, to track how it was spent, this and that?

Kornberg: I was chairman of that department, chairman here for 10 years. I don't recall that I spent maybe 5-10 percent of my time as departmental duties. And whatever time I spent had to do with someone who was having or creating some difficulties, or, beyond the department, as looking for new departments, influencing appointments in other departments.

Now it's very complicated.

Park: When was that \_\_\_\_\_?

Kornberg: It developed over time because the NIH has imposed strict accounting. Stanford then exaggerates that by, the auditors there don't care whether we do research. They care that they're not going to be guilty of not following the rules so that they overreact. Very difficult. And I rarely, maybe once every two years, incur an expense that I think my grant should cover, more than adequate \_\_\_\_\_. I have a gift fund that has no limitations with regard to how it's used. Stanford won't allow me to have business-class travel. It's as bad as the government.

Take this Taber [sp.] symposium, riddled with rules and limitations. So the government, almost by law, imposes restraints that are excessive. But look at the whole picture.

Park: Right. But in the 1950s and '60s, even '70s . . .

Kornberg: I don't remember \_\_\_\_\_. These things did accumulate with the increased budget and so forth.

But the extramural program had peer review, great. But I applied for a grant on a novel subject. There was nobody there who can review it. By definition, if it's innovative enough, there's no one on the study section who's had the \_\_\_\_\_.

But it's much worse in the European Union. There, you have to have several labs collaborate on a project, now maybe with industrial applications. It's a guarantee that it won't be innovative. It's assured.

Park: Actually, Dr. \_\_\_\_\_ mentioned the same thing. If you are creative as a scientist, and the project is really innovative, it's easy to get turned down.

Kornberg: And the paper would probably be turned down by the JVC \_\_\_\_\_, which happened to our papers on DNA polymerase. That's another story.

Park: In 19 . . .

Kornberg: Fifty--eight.

Park: Fifty-eight.

Kornberg: Because we called something DNA, and they thought it was presumptuous, and even though I argued that these 10 papers in the journal that had DNA didn't have criteria that it was DNA that even matched ours -- it had the polydeoxyribonucleotide -- \_\_\_\_\_ withdraw the paper. So, and they were eventually accepted, so . . .

So the NIH, as a human institution, has flaws. But eventually . . . And one of its major flaws is it's not supporting basic microbiology. Now it's worms and flies and mammals.

Park: But in the 1960s . . .

Kornberg: I think it was much more broad, tolerant in response.

Park: \_\_\_\_\_.

Kornberg: Yeah. You have to examine that. I don't know what measures you would use, but I mentioned the Yanofsky [sp.] professorship by the American Heart Association. It's not trivial. And the Cancer Committee of the American Cancer Society is called the Growth Committee. It was not any particular cancer. It was the Growth Committee.

Park: Speaking of cancer, there was an attempt by the Nixon administration in the early '70s, trying to . . .

Kornberg: Nineteen seventy.

Park: Yeah.

Kornberg: A number of us wrote a letter. I'm a signatory.

Park: Right.

Kornberg: To the crusade on cancer, something like that, the war on cancer.

Park: And I understand that you were deeply involved in \_\_\_\_\_ research in emphasizing that the basic research \_\_\_\_\_. Could you give me some comments on your experience at the time?

Kornberg: Well, again, there are a number of things -- and I don't know where they are -- and currently, one place or another, said that the genius of the NIH was that, in one way or another, recognizing that even the NIH itself was not planned, and that the best approach to doing something important in science was not have a plan. So mention penicillin, polio vaccine, anything, certainly, in physical science, MRI, x-rays, name something that was major, a major advance in medicine that was programmed and planned. I can't think of anything. So my point is that the accrual of knowledge -- physics now, mathematics is important in interpreting computer data, and chemistry, basic biology -- I don't think there was any major gap in translating important information. Everything now is about translation. So busy about translation, you don't even have a language to translate.

Park: That's right.

Kornberg: All said and done, I keep repeating, however it happened, however it's being done, the NIH has been -- \_\_\_\_\_ expansive -- one of the major achievements of Western government.

Park: How would you connect NIH's growth and its role in science to American science, American democracy, American system of learning new things, the American way of thinking, American culture? When you are thinking about democracy in America, how would you . . .

Kornberg: I don't know. That's your job.

Park: I am trying to get \_\_\_\_\_.

Kornberg: You saw my last slide I used in the symposium?

Park: Yeah.

Kornberg: The last slide. It didn't refer to individuals. This year is the 50<sup>th</sup> anniversary of the DNA structure and emotion and \_\_\_\_\_. The last slide. Let me see if I can find it. So, that's a tough question, and I think you're going to have to get an answer to it.

Park: You know, NIH has done a tremendous job in supporting science in the United States, but that there are other sources like industry and philanthropy foundations, and how do you compare NIH with other sectors, social sectors? No comparison?

Kornberg: Well, again, I've written it somewhere here at Stanford, and this has to do with what I'm concerned about more recently, that biotechnology, partnerships between universities and industry, have exceeded a point where it's balanced, and I think it's corrosive of basic science.

Here at Stanford, there was the Cohen-Boyer patent for recombinant DNA, which never should have been issued. It was a \_\_\_\_\_. And even Cohen, Stan Cohen, that patent shared by UCSF and Stanford over a period of 18 years, whatever a patent lasts, netted close to \$200 million, shared 50/50, and the Stanford Office of Science and Technology, 25 people, took its discount, 15 percent, whatever. You calculate how much and then . . . I'm sorry. So this income is shared three ways: Stanford, the Department, space medicine -- I'm sorry, genetics. It should have been biochemistry but it wasn't, and then the inventor, Stan Cohen. Let's assume -- this is excessive -- that half of that money -- it really should be more like a tenth -- half that money is spent on basic science, on research. We're being very generous; we're saying half. I calculated that it's just a little over 1 percent of what Stanford gets from the NIH. So here we have an office of 25 people promoting patents and royalties. Where is there an office for interpreting science that the NIH gets to do a better job in Washington? It doesn't exist.

So, how much does philanthropy contribute? You have the Howard Hughes Institute, some other institutes. It's catalytic. It's not substantial. At the most, maybe 5 percent of what the NIH \_\_\_\_\_, at the most. Industry, nothing. No industry invests in things that are immediately useful. \_\_\_\_\_. I mean, when I go to an industry, as I have on some occasions, and say, "Look, learning more about DNA or polyphosphate or whatever is really important to the future of polymers," biopolymers, whatever. Yeah, if you're right, Dupont in 1960 invest in nucleic acids. They'll tell you, "We are paying taxes. Those taxes are supporting the NSF and the NIH. We're doing our share." But to specifically sponsor a project . . . And when it happens -- and I'm complaining -- it gets to be so narrow both with respect to who owns the information, how long it's delayed in publication, how narrow it is, I think that whatever contribution there is ultimately is negative.

Park: I understand in 1980, Stanford biochemistry department studied the . . .

Kornberg: Industrial \_\_\_\_\_.

Park: \_\_\_\_\_ its program now. Was it successful in terms of both sides, industry and academia? Were both sides happy, have been happy? Is it still going on?

Kornberg: No \_\_\_\_\_.

Park: In the library, I looked at the list of companies that . . .

Kornberg: At that time, this department was the source of recombinant DNA and that technology. We were the leaders, or among the few leaders, and so \_\_\_\_\_ a few other companies, and, again, with some personal relationships, were members of industrial \_\_\_\_\_. And we helped because we gave symposia, personal advice to scientists in those industries, technical and otherwise. I think they got their money's worth. And we've had extra money without any restrictions of the NIH to support fellowships.

Park: It's kind of catalytic \_\_\_\_\_.

Kornberg: Yeah. But then it became so popular, and the biotech ventures actually have the science that came from Stanford. They didn't need us for maybe 10 years. And at that time, I think it was helpful to have that bridge, exchange, but now I don't think that . . .

Park: Do you think that the biotech \_\_\_\_\_ as applied science rather than basic science?

Kornberg: Of course it's \_\_\_\_\_. Hey, \_\_\_\_\_ biotech unless it's immediately relevant. No. Biotech -- and biotech ventures fail with rare exceptions -- have to be profitable or they have to be imminently profitable so that a big pharmaceutical company will buy them. No.

Biotech is useful. And, again, you've seen papers I've written about that.

Park: \_\_\_\_\_ all biotech.

Kornberg: About biotech. I've written about that, because biotech is doing something that you cannot or you'll do poorly \_\_\_\_\_. It's taking a discovery and producing it in the quantities that you can test its toxicology in animals and clinical trials. You can't do that with a discovery in the university. It's too expensive, distracting. We need \_\_\_\_\_. The pharmaceutical industry could do it, but they're very conservative now that they're getting to appreciate biologics, antibodies. No. Biotech still is a vehicle for, call it popular translation, translation of discoveries.

Park: Right, right.

Kornberg: I'm simply reflecting on where it's being overdone and distorted.

Park: I see.

Kornberg: And, look. No one of our graduate students would ever \_\_\_\_\_ industry. Now it's an attractive alternative. Does that reflect on the state of, the nature of our graduate students? We're getting graduate students who have the outlook of having a job ready and getting rich, and so they in turn are prepared to do genomics and industrial applications.

The few items I wanted to . . . How are we doing for time? Okay. \_\_\_\_\_.

I want to just mention these items that I jotted down \_\_\_\_\_. The peer review, as conducted by the NIH . . . NSF is another thing you should consider, because they've been more, less democratic, than the executive secretary, whoever, had much more authority. And so it's interesting to compare how is the NSF.

Park: There is a book out there about that. NSF \_\_\_\_\_ program. It's called \_\_\_\_\_ biology \_\_\_\_\_ history \_\_\_\_\_ basically all of NSF. And that program was closed in the mid-'70s, and it's exactly as you said. The executive secretary had more authority, power, to select the fields to support.

Kornberg: Okay. It's interesting, and I didn't know it existed or failed, but I know from personal experience that it was intrusive, objectionable.

Park: Right. It's very different from NIH's peer review.

Kornberg: Yeah, absolutely.

The NIH permits a larger group size and the flexibility in group size.

Park: Center grant?

Kornberg: Okay. That's a problem, R01 versus center grants. Center grants, almost by definition, are bad because the review process is taken out of the hands of a peer-review group, and it becomes university, local politics versus a governmental review, and university politics are usually much worse. It depends on individuals with different motives and power struggles. So I'd rather trust some indifferent government-selected group, even though they're not my peers, rather than the university -- a much broader range of subjects, salaries, and there's much more flexibility.

Park: What about the training program? You know, the NIH has supported medical students.

Kornberg: That's a good point. Medical students. Graduate students. Yeah. I think they've been excellent, excellent. Yeah. And it couldn't have done by, obviously, university is the recipient.

Park: And what do you think about trying to, more medical students to do research, science, and bridging the gap between science and medicine? Is it, has it been successful and it's going in the right direction?

Kornberg: I really . . . That's a tough one.

Park: What about the \_\_\_\_\_ experience of the medical students coming to the biochemistry lab and they learn something, and they later on appreciate what they learned here?

Kornberg: Early on, when the training programs were started some years ago, and there was some pressure to have a combined Ph.D./M.D., and I objected to that. I felt that if you had one degree, it was enough. In my case, I don't have a Ph.D. And you're imposing the rules and curricula that took precious time away from someone being creative, and eventually it was made clear to us that Stanford wouldn't get a training grant unless it adhered to this combined Ph.D./M.D. Now, is it possible for someone to do clinical research and be competitive in basic research? Give me examples of anyone. And I can give you a few examples out of constraint, anyone who is seeing patients, even if it's once, one month a year, attending, who's also doing competitive basic research. Very rare. And every time someone has an opportunity to escape a clinical obligation and wants to and can do basic research, they do.

And this now borders on another problem in medical practice, which is utterly out of hand. Medical practice in the U.S. -- and I'm told elsewhere, too -- chaotic. And we can go into that, but this isn't the subject. And maybe it is, because the NIH feels obligated to translate information from the bench to the bedside. Sounds great. And it should be done. And you're asking, can we train people, support and encourage them to do that? And I think it's increasingly difficult, not only to be a good doctor, think first about your patient, not about your grant application. Very tough.

I'm guilty of having preached for a long time, let's reduce the art of medicine and increase the technology. Now I have all this technology. People don't practice the art of medicine, using judgment: MRI, CT scans, whatnot, total blood chemistry, total hematology. If you don't do it, you might miss something. But in the course of doing that, expensive false-positives, impersonal attention to the patient.

One thing, especially since I was meeting with the Japanese people. Very early on, when the extramural program was inaugurated, '48, '49, '50, the extramural budget, modest, and I think I was clearly involved, but I don't know how influential, we would, priorities, list laboratories and include laboratories that were foreign. So if the -- I don't -- again, you can go back. We funded 20 percent of the grants or approved grants. I don't think we distinguished between domestic and foreign. Now, that was very strange because, I mean, with limited funds, just support the U.S. But by doing that and supporting postdocs, tens of thousands from Japan went elsewhere. It was a mini Marshall plan. We then supported science in struggling, emerging England, France, Israel, elsewhere. What happened? We flourished. We had the best brains. They came to us. English became the language of science. So among the things that the extramural program did was to foster and support, I should say rescue and foster science around the world.

Park: So when you were at NIH, the extramural officers asked for advice from you?

Kornberg: I think I was on the \_\_\_\_.

Park: \_\_\_\_ committee?

Kornberg: I was on the committee, member of a committee for a couple of years. And so they did, in the early years, recruit some of us to sit on it. I don't remember which and how much, but, yes, I was . . . But then the extramural program got enormous.

Park: That's true.

Kornberg: Now, you will have to, or should, as I said, contrast it with the Department of Agriculture. It's a great control. NSF. And, of course, the NIH exploited disease in a very intimate way and had constituency groups that pleaded for the NIH because it would help heart disease as well as mental diseases.

The other thing that the intramural program did, called the NIH shunt. I say interdisciplinary research was invented at the NIH.

Park: Interdisciplinary . . .

Kornberg: You know, it's a great buzzword now with the Okinawa people. Everything is multidisciplinary, it is collaborative and so on. But I think at the NIH, where the group size was small and specialists in protein structure and spectrophotometry, there were a lot of resources that you could go to, and there weren't clear lines that demarcated one kind of research from another.

Park: Was that especially the case when you went to NIH in the later '50s and '60s, do you think? Well, I found it. You know, here is one lab chief having the Laboratory of Molecular Biology, but the name doesn't really mean much at NIH. Some scientist has a laboratory of biophysics, biochemistry, but the name doesn't really matter. It's like a . . .

Kornberg: One of the reasons they could get away with it, you didn't have to teach a particular core course or discipline. So that pressure to teach biochemistry and to have members of that faculty or research group competent to teach biochemistry, so they could be chemists, they could be biologists, they could be . . . The NIH did allow that \_\_\_\_ intramurally and then extramurally. The study sections, of course, had to define disciplines. Here again, I think the breadth of activity extramurally -- first they expanded a number of study sections, so to accommodate what the science and the field needed. So again, a flexibility that universities could never do that.

Park: Did you have experience in the study section? Were you involved in any study section?

Kornberg: I served on a study section for a number of years. I'd say the \_\_\_\_ of the study sections were smaller. The scope of the science was small so that I think the quality of people on the study section was closer to peer review than later on. And then one of the abuses now, study sections are selected on the basis of gender distribution, geographical distribution, age distribution, all kinds of things, and now, I'm being told, political qualifications. So you have to fight it. But, still . . . And I've been turned down for a grant for the first time by a study section of bacteriology of . . .

Park: What year?

Kornberg: Two or three years ago.

Park: Oh.

Kornberg: And eventually doing something that, which to this day . . . You know my current work on polyphosphate.

Park: You mentioned a paper at a symposium.

Kornberg: Yeah. So I had to go back to the biochemistry study section to get the kind of sympathetic review that I've had all these years.

But all said and done, that last slide that I showed you.

I don't know what you're going to call your book. It's important that . . . Important. It would be desirable that it be popular rather than archival. I don't know how you do that. It's tough.

Park: Yes. I have to deal with science, politics, culture, and . . .

Kornberg: And it should not appear to be biased.

Park: No.

Kornberg: But it should be biased. How do you do it? I can write an article, NIH alma mater, the NIH, you know, but you can't do that.

Park: No, no. As a historian, no. Just like a scientist writing on their data, we are relying on our data.



Kornberg: I know. But you have to select the data.

Park: Yes, \_\_\_\_\_, and measure the significance of evidence here and there of which one is more reliable. And so, at the moment, I'm at the beginning stage of digging out the past voices in the archive and also from memory, and as time goes along, I guess the book should certainly sell. But at the moment I cannot . . . I have a . . .

Kornberg: Do you have a limit, a time limit?

Park: Well, it's going to be a three-year contract.

Kornberg: Starting . . .

Park: Starting this year. And I have already started, but I don't think it's going to be counted yet because I'm still in the fellowship program, so if the contract starts this year, then from then . . .

Kornberg: Three years.

Park: Three years. But I have collected a fair amount of sources from scientists like you, minutes of meetings, which is very reliable source, and also annual reports of each institute and articles written about NIH many times.

Kornberg: Of the personalities that you will feature, it's clear that you've selected Shannon.

Park: Oh, yes.

Kornberg: Who else?

Park: Of course you.

Kornberg: I don't know. It's exaggerated.

Park: Oh, no, no. I mean, at first, in forming the spirit of doing science after World War II, maybe a small group, and it's spreading around. And I'm \_\_\_\_\_ the only one. Your \_\_\_\_\_ is only one, but it represents very important spirit, best science, best research, going after that. And . . .

Kornberg: Do you think that what we did intramurally had an influence on the extramural program?

Park: Actually, that's what I want to find, the link, and your experience at Stanford, especially at Stanford, when you . . . But I take a look at \_\_\_\_\_ people, it's very much like NIH. It's very much, very like, very cohesive and also not touching many areas of biochemistry. Rather, specific area, but with various techniques, various expertise.

Kornberg: You're right.

Park: Physical chemistry, organic chemistry.

Kornberg: Yeah. You were criticized for being too narrow.

Park: But I guess it's very similar to NIH, \_\_\_\_\_. So if I can't find a link, your case is one example. But I want to see more general way of in terms of program influencing outside programs, \_\_\_\_\_ programs.

Kornberg: That's no longer true.

Park: Yeah.

Kornberg: The NIH shunt, the yellow berets, clearly, that was metastatic, and . . . But I don't know that that's been true in recent years.

Park: I guess it's . . .

Kornberg: I'm not denigrating the intramural research, but it is a fact that there isn't the same kind of pressure and evaluation, and some of that's very good because someone intramurally has a lot more freedom. I don't know. These are tough. Well, historians traditionally have a point of view.

Park: Oh, yes.

Kornberg: And if anyone disagrees, let them write their history.

Park: That's true.

Kornberg: But one thing, if I had that job -- it was suggested to me I should do it, and, of course, that's absurd. I couldn't bring the time and scholarship any more than I could to write a novel. But the point of view that I try to persuade people is that one, by some route, you may not \_\_\_\_\_, something was done that was remarkable by the government. People say, "Get the government out of it."

Milton Freedman [sp.]. Do you know his name? He's a famous economist.

Park: No.

Kornberg: You don't. Very famous economist. He probably got a Nobel Prize \_\_\_\_\_, a very conservative person. By . . .

Kornberg: You should know his name because he's a very well-known conservative economist. At a dinner party, he asked me what I was doing, and somehow I made some point of how much the NIH is doing to support science, my science. \_\_\_\_\_ at Stanford \_\_\_\_\_ all over the country and the world. And he was really \_\_\_\_\_ initials \_\_\_\_\_. And, in fact, that's generally true. If you poll the public, how many people in the community will know what NIH \_\_\_\_\_. They know probably FDA, and certainly IRS, but NIH doesn't have that recognizable status. I described \_\_\_\_\_. He listened to me briefly and said, "You know, that should be privatized." It was, you know, there are stupid statements, and that was certainly one of them. I didn't know what he was talking about. And so the mission of a history like this ought to be, this is an illustrious achievement of government, federal government. No state could do that. And how successful it's been. So I think one of the \_\_\_\_\_ that deserves emphasis: This is what the federal government has done.

You should quote Lou Thomas. Will you? Very gifted, and he's . . . Someone sent me the Axelrod recollections. Do you have it? Yeah. And he . . . It's not well written, but he quotes Lou Thomas. Yeah. Someone like Julie Axelrod, very impressive example of what the intramural program can do.

Park: I was going to ask you about, NIH's great achievement was made not only by bureaucrats, administrators, but by scientists, scientific community, who voluntarily run the system. The peer-review system is actually the scientific community's reviewing \_\_\_\_\_ in a fair way. How do you . . . You know, there is a . . .

Kornberg: I don't -- look, I don't give scientists any special \_\_\_\_\_. They're on a peer-review committee because they're expected to be, gives them some status, some conscience about doing a good job. But who organized the peer-review system, the study sections? Who got the money? Scientists couldn't have got the money. They helped. They appeared before Appropriations Committee. They did things that were demonstrably useful in coping with disease, responsible for an industry called biotechnology, the pharmaceutical industry. No, I wouldn't give the individual . . . The individual scientist is more a beneficiary than a donor. Because I don't think American scientists are any better that way than the Japanese or the English or the Italian. They are fortunate to have been born in America and have access to the system.

Park: As you mentioned, it wasn't really planned in the beginning. It evolved into a very fine system.

Kornberg: Exactly.

Park: And one -- someone may see American democracy as a plural system. It's not one system; it's many funding sources, many government sources. At a glance, it may look very hazy and not working very well, but we have NSF, NIH, Atomic Energy Commission, those other departments, and the sources. And how do you, you know, not only just NIH as one thing, if you think America bureaucratic system . . .

Kornberg: Well, it certainly is not a centralization of science.

Park: Right.

Kornberg: And the universities, unlike Japan, are not centralized. They're free to go their own ways and fail and compete. And so in my evaluation, in terms of the support of science and scientists, what has Stanford, Harvard, or any other university done? They benefitted by indirect costs. They had these gifted scientists teach the courses. And in most cases, they actually laid out far less money than they've received. However, each one has a capacity to act on its own. If you don't like it here, you can go elsewhere. If you go elsewhere, why did we let this good guy, this money magnet, escape us? So I don't know whether the NIH could have worked in Italy. It hasn't. I mean, every attempt in France, Italy, Japan, hasn't worked. So that's another thing for you to do. What is there in the political system, the culture, that's made it possible for this to evolve and grow?

Park: How can you compare NIH with British counterpart?

Kornberg: That's the point. You should do that. I would say that you can't find any equivalent anywhere else.

Now, examine England. I think that the research councils, what the volume or the amount of what they support is far less, and how democratic or fair and skillful is it? Yes, I think that it is still more of the old-boy top-down than the American system.

Park: I don't know whether it is a fair judgment or not, but I read an article written in the '60s or '70s about the peer-review system. It is talking about young scientists who don't have much \_\_\_\_\_ research tend to get resentful, whereas \_\_\_\_\_ scientists tend to get more, easier way \_\_\_\_\_. Was that a fair . . .

Kornberg: \_\_\_\_\_, you know. It has some correction in that the study sections tend to be younger, and they reject that kind of elitism and reputation-driven qualification. Look, if you have to invest in a project, unfortunately, they're projects, ideally would be in people: I'm going to give this money to Arthur Kornberg because he's demonstrated these capacities and achievements. Let's give him this amount of money for so many years. I'm not going to ask him what he's going to do with it. It's an exercise. And when I was on a study section, I think I had some influence in saying, how are we to judge this eminent biochemist who's not written a good grant proposal? And it's foolish. We're not judging him on his capacity to write a grant proposal. We're judging him on the basis to continue the eminent science he's done. Let it go at that.

How often does a member of a study section speak up to the person who's assigned them a reviewing responsibility and say, "Look, lay off it. This guy's a great scientist. He's done more than you or I will. Who are we to deny him the support that he's earned and deserves?" and let it go. To ask him to revise it is just an exercise . . . I don't -- I would say unproductive.

To some extent, that applies to publishing papers. There are editorial reviews that are very strict and unnecessarily demand that standards, certain artificial standards be observed. I think the NIH study sections, when they are being very strict, demanding, probably are counterproductive. But that's tough to say. Who in this list of people do we judge on their achievement? I would say, broadly -- look, my proposal a long time ago is when someone who's qualified by virtue of a degree, you know, respectable degree, done a postdoc, give him a grant. I don't care what he asks for. Come back periodically, three, five years, and say, "What have you done? You really haven't published much," and he was tackling a very difficult subject, and the data he was collecting were as good as one could expect \_\_\_\_\_. Here's another. But Congress, I'm told, demands that there be a proposal and a time scale.

Park: Well, what \_\_\_\_\_ intramural program is that the intramural scientists don't have to write grant proposals and they can do long-range project, which is a bit difficult to be tackled by extramural scientists. Is that true?

Kornberg: Well, I mean, in the best sense, yes. But are they being judged by the same standards that people outside \_\_\_\_\_? No, I don't think so. Are there excellent scientists at the NIH who benefit from this kind of freedom to pursue a subject, difficult, unpopular, \_\_\_\_\_? Yes. Is it true of all the science at the NIH? Obviously not. But overall, just in the support of science and the training and growth of scientists, the extramural program is what is remarkable.

If you wanted to be very either courageous or foolish and say we've got to give up one or the other, there's no question that you'd give up the intramural program.

Park: The intramural program is like 10 percent of the \_\_\_\_\_. Is it 90 percent . . .

Kornberg: It's usually more.

Park: Well, even in the 1960s, intramural is 20 percent.

Kornberg: Twenty percent. That's right.

Park: And these days only 10 percent.

Kornberg: Yeah. So I would protect it because it's \_\_\_\_\_. And for reasons you just gave, that people have small groups and long range and are competent and they're doing work . . . And there is a review. If someone is not doing much, their lab is taken away from them. Isn't that so?

Park: Yes. Have you been involved in reviewing \_\_\_\_\_?

Kornberg: No. I may have some \_\_\_\_\_.

Park: Have you been involved in reviewing NIH in general? From time to time, there is reports about NIH, how it's managed . . .

Kornberg: I don't think so. No. Have I been called on to do it and refused? I don't remember. I've not been as active in extracurricular affairs. I've written; I've done some writing. But I've not been on major review committees or advisory . . . Well, I've been on some. This Okinawa thing, for example. I have some special interest in Japanese. I have maybe 30 Japanese students and postdocs. I've visited Japan often. I'm not a Japanophile, but . . . Well, today I was going up . . . You guys ought to be recruiting from Korea and China. That's where there's a lot of talent. That's where Okinawa can get some of its best people.

Park: There are some Koreans going to Japan \_\_\_\_\_. There is \_\_\_\_\_. Osama Hyishi [sp.] still active in Japan?

Kornberg: Well, retired from being the director of the institute. I haven't seen him for a while. We're very close friends, but I haven't been in Japan for a few years.

Park: Before closing . . .

Kornberg: But you should, if you can. He would be a good one to talk to.

Park: Oh, yeah.

Kornberg: Or to correspond with. He's not as open and free with . . . Very reserved, likely to not say anything critical or \_\_\_\_\_. But on the specific issues, it would be very worthwhile because . . . What is the Japanese system for the support of science versus the American system? It's a huge difference. But he could give you angles on it that may be helpful.

Now, tell me, with regard to the Stackman [sp.] exhibit, I'll be preparing a talk, and it would be helpful to me to know in some general way what's in the exhibit, either to respond to it or not to repeat.

Park: Well, sure. First of all, I will send a script to you by e-mail. Is it okay? The script of the exhibit where I describe why the Stackmans [sp.] came to NIH and what areas of science they \_\_\_\_\_ and what kind of people they trained at NIH.

Kornberg: They were very high on Koreans.

Park: Oh, yeah? Yes, that's right.

Kornberg: They had a colony.

Park: Just like you have a Japanese.

Kornberg: Yeah, yeah. \_\_\_\_\_.

Park: And so, that's just three major things: why they came to NIH and their . . .

Kornberg: Do you have the . . .

Park: I have photos and I have pictures, and . . .

Kornberg: Can I have Harker's [sp.] recollections?

Park: Yes, and your review.

Kornberg: And of course you have Earl Stackman's [sp.] reflections \_\_\_\_\_.

Park: Yes. And Terry also.

Kornberg: Yeah, okay. Please help me with things that I can say about Terry, because I have Earl's recollections, Barker's reference to, largely to Earl. I don't think there's any reference there to Terry.

Park: Terry wrote own reflection.

Kornberg: For what?

Park: For her science. It's in your review of microbiology \_\_\_\_.

Kornberg: Okay.

Park: And I can give you . . .

Kornberg: You can give me the reference.

Park: Reference \_\_\_\_.

Kornberg: I'd be happy with that.

Park: And there she described her scientific \_\_\_\_ in detail.

Kornberg: Incidentally, Gonzales \_\_\_\_\_. Is Gonzales alive and . . .

Park: Yes, I think so.

Kornberg: So far as we know.

Park: Yeah.

Kornberg: Okay.

Park: So you may want to contact Gonzales, too, because Terry's relationship with Gonzales was very strong, as strong as Bob Cohen.

Kornberg: Terry and Gonzales.

Park: Yes. And Terry's main research area is, as you know, in vitamin B12 metabolism \_\_\_\_.

Kornberg: Yeah, but I don't want to repeat things that are clearly in the exhibit. I'm not sure what I should talk about, but clearly, on a personal level, what, having been together at the NIH, the influence that they had on, well, going to work with Barker. Yeah. So that'll be there, but it's too historical and really just repeats what's in the exhibit. I don't think there's much point in . . .

Park: But it should be very nice if you can say a few words about their position in the history of biochemistry, how prominent they are and what you're thinking of the promise of biochemistry in the United States and where they fit in and . . .

Kornberg: The exhibit will try to do that.

Park: Will try to do that, but that's historian's view. You know, here is a prominent biochemist saying about \_\_\_\_, kind of is different weight, I guess \_\_\_\_ my interpretation of their contribution.

Kornberg: Have you prepared the text for the exhibit?

Park: Yes. And I will send you . . .

Kornberg: What does Buhm Chuck [sp.] do?

Park: Support me.

Kornberg: I just wanted . . .

Park: And, actually, related to Art and Terry's work, could you say a little bit about enzymology? We discussed that before our interview. There are a lot of \_\_\_\_ these days, but as a \_\_\_\_.

Kornberg: Yes. Please jot those things down and they'll be helpful to me. Yeah. I appreciate it.

Yeah. I traveled to the Taber [sp.] symposium, to the Stackman [sp.], and it's a matter of personal affection. And is this exhibit something that's been done before for others?

Park: For others? Well, this is the first one that I did, and there is . . .

Kornberg: Now, is it your initiative to do something like this?

Park: Well, actually it was Buhm's initiative and \_\_\_\_ initiative, and I happened to be there, and so basically they recruited me in the fellowship program \_\_\_\_ doing the job. But there are similar participants, like Martin \_\_\_\_.

Kornberg: You know, it's gotten late and I haven't even offered you a drink. Would you have some tea?

Park: Well . . .

Kornberg: Okay. Let me ask my secretary.

[recorder turned off]

Park: . . . NIH, there are some other, because if it's \_\_\_\_ happen to see that the Clinical Center is Martin Broadbury [sp.] exhibit; there is a \_\_\_\_ exhibit; and there is an \_\_\_\_.

Kornberg: These are permanent exhibits?

Park: Should be, but at the moment, the Clinical Center is in the process of renovation and there is new Clinical Center. There is a current one; there is a new building adjacent to the current one, and it's going to be opened next year. So at the moment, there is a sort of renovation going on, refurbishing going on. So the Stackman [sp.] exhibit will be \_\_\_\_\_ in the \_\_\_\_\_ auditorium, one of the \_\_\_\_\_ Clinical Center, near that, but it's not the permanent place. And when all of the dust settles down, it will move to the permanent location. So it's designed to be a permanent one.

Kornberg: What building?

Park: In the Clinical Center.

Kornberg: The new Clinical Center.

Park: Well, probably old one.

Kornberg: Old one. And the old one, will it have any beds?

Park: Yes, I think so.

Kornberg: It'll continue to have beds.

Park: It'll continue to have beds, but the new one \_\_\_\_\_.

Kornberg: The new one will also have . . .

Park: Have beds, but not as many as . . .

Kornberg: And in size, will it be comparable to the old?

Park: Yeah, comparable to that \_\_\_\_\_ another opening of the Clinical Center.

Kornberg: Yeah. So it'll be very helpful to me.

I suppose the lecture will then be given in that same auditorium where the Taber . . .

Park: Yes, yes.

So, do you have anything to talk about NIH or . . .

If you allow me, if you're not too tired, I want to ask you about the field of biochemistry in general, because I'm interested in the history of biochemistry in relation to microbiology, biophysics, and other things going on. And the \_\_\_\_\_ is very interesting because you have a great influence upon nearby departments like chemistry, biology, in terms of selecting key scientists.

Kornberg: Not that much. I've mentioned already that among these commandments, which is a device to attract attention. I make it very clear early on that each of us has his own 10 commandments, and in this case I'm amending the 10 commandments. So I've said that the focus has shifted from enzymology -- which is not replaced by genomics or proteomics; it's wet; it's truly functioning -- and the importance of microbiology as a means to do biochemistry, and, of course, genetics.

I'm aware that this kind of focus has diminished. My son Roger, who does this very eminent three-dimensional structural biochemistry, and has the choice among the best young people, postdoctoral work, is finding that he has some very gifted crystallographers, but not the biochemists, or they're not as numerous and outstanding. So . . .

Park: The reason I'm asking this question is, in the history of science, there is a growing interest in the history of microbiology. And in that literature, the contribution made by biochemistry, biochemists, is a bit set aside.

Kornberg: I'm aware of that.

Park: Yeah. For example, the story of Watson and Crick that other \_\_\_\_\_, there is a main \_\_\_\_\_ has big events going in the '50s and '60s, on into the '70s. But the story of people like you or other enzymologists, biochemists contributing to \_\_\_\_\_ are not really well \_\_\_\_\_. The only biochemist who is really mentioned is Shargaf [sp.] as a person who \_\_\_\_\_.

Kornberg: Well, I've commented on that many times, as you probably know, that . . .

Park: But as a field of enzymologists, biochemists, it's not really well studied by historians as well as the scientist.

Kornberg: I don't personally feel neglected. But I would agree that the glamor -- and Watson deserves a lot of the responsibility for that. You know, despite his awkward personality, he has glamorized that double helix, everything that's done with DNA before and since. You know, it takes a publicist, an exponent, to shift attention. And Shargaf [sp.] is his own worst . . . But as I point out, people have urged me, he should be proposed by the Nobel Prize, or I'm proposing him, would you support it, and my response is, Shargaf [sp.] not only didn't discover \_\_\_\_\_, he rejected it years after it was clearly true. He deserves credit for recognizing the work of Avery and \_\_\_\_\_ McCarney [sp.] and recognizing how important DNA was, and then did analytical work that showed the equivalence of purines and pyrimidines. That . . . But he is not the biochemist who understood how DNA was made and rearranged and repaired. Not very great \_\_\_\_\_, but . . .

Park: Could you say something about Herman Kalckar who actually came to NIH and spent some years at NIH, and his wife also came to NIH, Barbara, second wife.

Kornberg: I know.

Park: And was he hoping to work with you?

Kornberg: No. Herman Kalckar, like Ochoa [sp.], was one of the main figures who recognized the connection between or the need to understand how energy was captured from combustion of fuel and sugar, and in the '40s, attracted attention to that problem and did something about aerobic phosphorylation. A lovable guy.

I'm trying to think of Barbara's last name.

And it's interesting that Danish biochemistry had a brief flash of visibility.

Park: Was he -- did he continue to a good job, good work in the United States after coming to . . .

Kornberg: I'm not aware of it. I don't know.

Park: Do you remember Gordon Tompkins? You said that he applied for a position in your lab.

Kornberg: Postdoc.

Park: Postdoc. But eventually he became lab chief of molecular biology at NIH, and later on . . .

Kornberg: Went to UCSF.

Park: UCSF. But he tragically died early. Was he a very prominent biochemist of his time?

Kornberg: I gave the Gordon Tompkins lecture, I guess this spring. Maybe while you're here, my son called me to tell me that the honorarium for that lecture, which had been lost, was finally retrieved.

Gordon's achievement, the most prominent, is that he and Bill Ratter [sp.] rescued an obscure department and created one of the major centers of biochemistry, biophysics, and so forth. And I confess that I can't give you the kind of assessment of what Gordy did. Very bright and engaging and charismatic. Look, we can play a game of who did what when and how this stands up to a long-term series of achievements, and it would be unreasonable. Did he do as much or more than Marshall Nirenberg or someone else?

Park: Speaking of Marshall Nirenberg, you were interested in hiring Gordon, his name . . .

Kornberg: Corona.

Park: Corona, Gordon Corona.

Kornberg: We're very close friends.

Park: Yeah. You wanted him to come to chemistry department at Stanford, and he . . .

Kornberg: I've forgotten. It could have been.

Park: Could have been.

Kornberg: Great asset, great appointment. I'm sure I don't mention it in this essay on the two cultures. The three departments were not disposed to hire or even appreciate a Gordon Corona. He used enzymes in his syntheses, and you don't do that.

Park: So biochemistry department is much better off in the medical school than \_\_\_\_\_?

Kornberg: Oh, that's a matter of local politics. Is Stanford University better off with a medical school than not having it? That's still a central issue, how hospitable a university is to a medical school and its hospital and budget and distractions. That's fought over to this day. But in the history of science, especially because biologic science is now viewed as the science of the decades or, and the medical component of bioscience, biochemistry, physiology, and so forth, genetics, and I think that's a question that you have to think about and evaluate.

Park: Well, I guess at this point I can stop the tape recording. Do you have anything to say?

Kornberg: No, not really. You've done a lot of homework, and I want to help you as much as I can because I think the impact of a good history of the NIH will be considerable. And how you present it -- the title, the approach, the readability -- very important.

Park: Thank you very much.

Kornberg: Right. Thank you.

Park: It's a big pleasure.

END OF INTERVIEW